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ABSTRACT

This paper deals with future research needs and problems in learning disabilities, and is divided into the following two broad categories: (1) supporting conditions, which involve necessary prerequisites to the research effort; and (2) procedural considerations, which deal with methodological concerns. First, the problems posed by supporting conditions such as financing, training research personnel, gaining cooperation from practitioners, and maintaining research's credibility are dealt with. Then, the second set of research needs, those involving procedural issues, are discussed. Two basic procedural needs are discussed. The first involves the fact that research in learning disabilities relies on established research paradigms in an effort to test competing falsifyable models. The second involves finding ways to bridge the gulf between research and the unknowns that the practitioner must face daily. The paper maintains that the basic research problem confronting learning disabilities (or minimal brain dysfunction) is that of sample definition. The final section of the paper discusses categorization of disability (minimal brain dysfunction, learning disabilities, culturally disadvantaged deaf, blind, etc.), the use of these labels for administrative purposes, and the reaction to them by educational practitioners. This section also discusses the problems caused by categorization in classroom management, remedial programming, administration, and research methodology. (BD)

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The staff of the LTI-LD, however, feels that the contents of this Preview Series are significant enough to warrant the attention of professionals concerned with the education of the learning disabled. We sincerely hope that this series is relevant to professionals and that it will be shared within the educational community.

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Future Research Needs in Learning Disabilities

Gerald M. Senf[2]

For convenience, I have divided the research problems I shall discuss into two broad categories, supporting conditions and procedural considerations. The former involves necessary prerequisites to the research effort while the latter deals with methodological concerns. While I find myself more intrigued and challanged by the procedural issues, the supporting conditions place definite research restrictions on the research effort and are very much as important.

I have become particularly aware of the problems posed by supporting conditions such as financing, training research personnel, gaining ccoperation from practitioners, and maintaining research's credibility during the past year or so. Being on a leave of absence from a research and teaching position at the Psychology Department of the University of Illinois, Chicago, I have been working with an applied special education project called the Leadership Training Institute in Learning Disabilities based at the University of Arizona's College of Education. This position affords me some contact with the federal funding source for learning disabilities (Bureau of Education for the Handicapped, USOE) and nation-wide consulting experience with learning disability service programs.

It has been an eye-opener for a psychologist contributing to the basic research literature in learning disabilities (LD) attending conferences of the Association for Children with Learning Disabilities and the Council for Exceptional Children and associating with prominant figures in the field of special education. In general, I do not see much fertile ground for research.

Nowhere is this opinion more fully borne out than in the area of financing. While service and training programs have also felt the pinch of meager allocations from the Learning Disability Act of 1969, research got nothing. I know of only one BEH supported project involving learning disability research, itself a test development program. While LD monies were admittedly meager, it is significant nevertheless that a policy decision allocated all of the funds for service programs, reaching into other funds for teacher training programs, but finding none for basic research. The Research Branch of BEH historically has supported broad scale institutions such as Regional Resource Centers rather than adopting a funding model I would, that being competitive grants to individuals typically housed in institutions of higher education where research training is a natural byproduct of the research endeavor.

Without research grants, training of research personnel in Learning Disabilities at College of Education becomes a nearly impossible task. Though my evidence in this regard derives from a limited range of experiences, it is my impression that Learning Disability programs do not attract students with research interests nor are they staffed by professors with active research programs for the most part. One must remember that teacher training is the primary function of Colleges of Education. The problems involved in promoting exemplary research are similar to those faced by clinical psychology; the students typically have applied interests and the staff's strength frequently lies in theory and practice rather than research. Unlike clinical psychology, learning disability lacks the host of research competent colleagues within their own department who can impart research skills to the clinical students and try to instill in them the skeptical, inquiring approach

of the scientist. Without the intellectual discipline and scientific method of inquiry provided by research minded colleagues, the practitioner-teacher can readily fall prey to unverified clinical wisdom or much worse to a cult of personality. Clinical psychology has surely had to weather the whole range of these problems nor have the experimental fields totally escaped the detrimental effects of powerful schools of thought or of personalities. Learning disabilities and education generally has, in my opinion, been particularly vulnerable to the hasty application of untested theories and to the domination of a line of strong personalities. The result is that accepted practice relates more to political power or influence rather than to the accumulation of knowledge. I view this problem as very serious as I do not see how the field of learning disabilities will ever come to train researchers given the present climate. This second resource problem, that of research personnel, is very critical.

My own belief is that research-trained persons from allied disciplines will accomplish the relevant basic research as, in fact, they have been doing right along. The problems here are that researchers lacking intimate knowledge of learning disabilities may not ask very cogent or sensitive questions. Given our present level of understanding, I believe that better hypotheses will derive from experience with the phenomenon (LD children) than from reading the research literature. With this in mind, I always require graduate students wanting to do a thesis on learning disabilities to work with LD children in a clinic or special education setting. The uncertainty and anxiety level of the student may be higher as a result but the thesis question asked is typically not trivial.

The second problem attendent upon having non-educators accomplish the needed research involves funding. Funding agencies will have to become more open-minded vis-a-vis their grant recipients' affiliations. Competitive

grants, judged by multidisciplinary committees, are a necessary step in buying the best research for our money.

A third problem is that of dissemination. How does research accomplished by psychologists, physicians, and scientists allied to other professions become part of the practice of special education? The ramifications of this question are simply too broad so I shall just pass over it.

Cooperation of practitioners is a third supporting condition that creates problems for research in learning disabilities. The simple mechanics of obtaining suitable subjects for research studies requires an investment in time and energy far in excess of the value of the knowledge typically gained. Schools seem more protective of their children's time than do parents! From the point of view of the school, it is easy to see how disturbing to everyday procedures a researcher's presence can be. Typically, schools receive nothing but headaches from researchers, unless of course the study finds significant resu[illegible], and then the thoughtful researcher affords the school principal a footnote. This practice relationship, which is exhausting to both parasite and host, needs reexamination.

Researchers must consider alternatives which might enhance their reception. The "hit and run" tactic, while acceptable for producing publications, cuts one off from valuable longitudinal data and individual case studies that are so badly needed. A number of alternatives come to mind: researchers can provide schools testing services often required or useful for the research itself; they can conduct an in-service training session or two for the school's special education staff or for all teaching personnel on any variety of topics, e.g., testing, research methods, one's own research program, review of research findings. Involvement of graduate students in

such endeavors provides them valuable experience as well as an entree for their thesis research. The researcher with more applied interests and a flexible budget can enroll students for course credit and train them to provide tutoring to needy children, thereby creating a research sample which allows for continuous assessment. Alternatively, the researcher could associate with persons with applied interests and conduct his research under their aegis. The research I conduct this year follows this model. The general point here is that we must become more atuned to the problems in one-shot research and learn how to develop more lasting, mutually profitable relationships with practitioners.

The fourth supporting condition, the credibility of the research enterprise, underlies the other supporting conditions. Campbell and Stanley, in their widely known book on research methods in teaching (1963) place the credibility issue in a historical perspective. Their commentary, written more than ten years ago, is even truer and perhaps more timely today: They wrote:

> "This chapter is committed to the experiment: as the only means for settling disputes regarding educational practice, as the only way of verifying educational improvements, and as the only way of establishing a cumulative tradition in which improvements can be introduced without the danger of a faddish discard of old wisdom in favor of inferior novelties. Yet in our strong advocacy of experimentation, we must not imply that our emphasis is new. As the existence of McCall's book makes clear, a wave of enthusiasm for experimentation dominated the field of education in the Thorndike era, perhaps reaching its apex in the 1920s. And this enthusiasn gave way to apathy and rejection, and to the adoption of new psychologies unamenable to experimental verification. Good and Scates (1954, pp. 716-721) have documented a wave of pessimism, dating back to perhaps 1935, and have cited even that staunch advocate of experimentation, Monroe (1938), as saying "the direct contributions from controlled experimentation have been disappointing." Further, it can be noted that the defections from experimentation to essay writing, often accompanied by conversion from a Thorndikian behavorism to Gestalt psychology or psychoanalysis, have frequently occurred in persons well trained in the experimental tradition.
>
> To avoid a recurrence of this disillusionment, we must

> be aware of certain sources of the previous reaction and try to avoid the false anticipations which led to it. Several aspects may be noted. First, the claims made for the rate and degree of progress which would result from experiment were grandiously overoptimistic of nonexperi- accompanied by an unjustified depreciation of nonexperi- mental wisdom. The intitial advocates assumed that pro- gress in the technology of teaching had been slow <u>just because</u> scientific method had not been applied: they assumed traditional practice was incompetent, just be- cause it had not been produced by experimentation. When, in fact, experiments often proved to be tedious, equivocal, of undependable replicability, and to confirm prescientific wisdom, the overoptimistic grounds upon which experimentation had been justified were undercut, and a disillusioned rejection or neglect took place."

As I noted previously, the field of Learning Disabilities acts as though it concurs with Monroe's disillusionment in failing to promote the research necessary to validate the basic principles upon which their remedial programs are based. At the same time, the opinion that research has not brought us much has merit. Too often, researchers argue that the limited practical value of research is due to its "basic" nature and that benefits will eventually ensue. I frankly do not believe that most researchers dealing with psychoeducational phenomena can credibly, nor validly for that matter, claim that theirs is "basic research" which will some day contribute meaningfully to educational practice. All too many of the studies in journals make me wonder where the author thinks this study will lead him except up the professional ladder. I believe that researchers too often reject the clinical wisdom of the practitioner, preferring to minuet while the practitioner fights the battle. The image of research is certainly not aided by the overwhelming number of one shot theses and dissertations that crowd the literature. Programmatic research, funded over extended periods of time under the direction of skilled and dedicated scientists is needed. I know from my discussions the last couple days that

many of the speakers at this conference share this view and would welcome the funding to make possible such long-range research effort.

To foreshadow something I shall develop further later, I believe that our credibility as researchers depends heavily upon producing usable information and ideas. I am in total support of scientists whose research has no immediate or even obvious potential relevance so long as there exists some rationale for the investment of tax dollars, granting that these distinctions are often terribly hard to make in practice. I do believe that the "basic research" argument is very often offered as a foil, an excuse for the researcher's failure to ask incisive questions or to discover and utilize a methodology that will advance knowledge. While it may appear that I am impugning the integrity or at least the proficiency of many psychoeducation researchers, my motive reflects my understanding of the problem from the practitioners' viewpoint. I agree with Campbell and Stanley concerning the "unjustified denunciation of nonexperimental wisdom" and wish to acknowledge publically that the "research emperor's clothes" may not be everything we have been taught to believe. The less we as researchers hide behind the "basic research argument", the more likely I believe it will be for us to form more cooperative and productive relationships with practitioners, to see and respond to the challenge of tougher but more meaningful questions, and to regain once again the credibility and hopefully the tangible support for the research we know has to be done.

I shall turn now to the second major set of research needs, those involving procedural issues. The first procedural need is that research in learning disabilities rely on established research paradigms in an effort to test competing falsifyable models. The second need involves finding

ways to bridge the gulf between research based knowledge and the unknowns that the practitioner must face daily.

How many times have you read a research report characterized as follows: first it is alleged that LD children have an"X" deficit. A test or scale is constructed to measure X. A sample of LD children are compared with some contrast group and found to be deficient in X. It is concluded that LD children have X deficit. A major variant on this approach is to adopt a previously existing test of X which likewise lack the necessary construct validity to make conclusions truely "meaningful" in the logical positivist sense of the word.

My argument with this type of research, which I contend is plentiful, is threefold. First, creating a measuring scale de novo represents the extreme case of adopting an unvalidated test. One simply does not know the empirical correlates of the measure. One may find that it distinguishes LD children from some contrast group (typically called normals) but that is all one knows. Further assertions, which there always are, rely on the face validity of the scale or upon the author's assertion of what good the scale measures; wanting to measure X, the researcher creates a scale that ought to be (he thinks) a measure of X (or he selects one that other persons have said measures X.) Finding that the scale differentiates LD from normal children, he asserts that LD children have disability X. He does so on faith in his own test construction abilities or in that of others. The study is basically meaningless, unless one wants to join in the author's guessing game. I term this type of study the "simple sample difference approach utilizing unvalidated (meaningless)instrument."

It is mandatory that instruments used in research adopting this type of design have established meaning. Such meaning could be the result of test validation or could derive from a body of research utilizing the test

paradigm in research studies. I find this latter procedure, the use of well-researched experimental paradigms, to be potentially most fruitful by virtue of the power available through making small yet critical variations in the experimental paradigm. Dr. Marshall's paper presented earlier illustrates this point. His conclusions rest on our knowledge of the serial position curve and attendant theorizing. It is only from this knowledge base that he can assert that the LD children in his sample evidence greater long term memory problems on the average than his controls. Had he created his own test of long term memory (and not undertaken the lengthy task of validating it as a measure of long term memory), his paper would have been worthless in my opinion. On the contrary, his use of a well-researched experimental paradigm is exemplary for it has allowed him to interpret his results within the context of existing knowledge.

My second quarrel with research using the "simple sample difference approach", i.e., comparing LD children with normals, is independent of whether the test instrument(s) adopted has pre-established meaning. Predicting that LD children will perform more poorly than age and IQ controls is a pretty weak (though "safe") hypothesis. The fact is that LD children, like juvenile deliquents, psychopaths, and physically handicapped children, will perform more poorly than controls on almost any task given them. Their test taking abilities and attitudes together with their history of failure result in poor performance throughout. Consequently, the meaning of a difference between the score of an LD child and the normal control may not have the same meaning as the difference between the same two scores obtained by normally achieving youngsters. Said somewhat differently, the construct validity of the score may not hold for the disabled sample;

method variance may obscure the content variance. This same problem occurs in practice where diagnostic testing may find the child to have 10 or more psychoeducational disabilities where in fact he may have a single problem, an attentional deficit for example, which prejudices his performance on all tests administered. Aside from the fact that positing multiple deficits is not parsimonious, it is exceedingly perjorative unless one can substantiate the construct validity of each deficit purportedly identified.

One solution to this problem is the utilization of falsifyable models which are tested using with-child data or at least comparisons between groups of LD children, not between LD and normals. Again, Dr. Marshall's study is an excellent example; the more important point was not that LD children performed worse then the normal children, though they indeed did on the average, but that the within subject data indicated that the long term memory (primary effect) rather than the short term memory (recency effecy) showed weakness. Had, in fact, the LD children simply performed more poorly overall, no incisive conclusions could have been drawn.

The notion of a falsifyable model is that assertions exist for which invalidating data could potentially be found. The "simple sample comparison experiment" can only determine that the hypothesized difference exists or that the difference was not found (but not that the difference does not exist).

It is more efficatious to design experiments which pit explanatory alternative against one another. A timely example derives from the work of Professor Barbara Keogh from UCLA though I shall likely not be faithful to the details of her work. One could assert that the hyperactive child selects inappropriate cues thereby making his behavior maladjusted. Or one

could assert that the hyperactive child selects the same cues as the normal child but is inordintly rapid in acting, poor judgement being the essence of his maladjusted behavior. Parenthetically, it is interesting to note the diverse practical implications of these two hypotheses. The first, in positing a cue selection disability, would imply the need for special educational materials tailored to restrict task-irrelevant cues or suggest the need for distraction-reducing work cubicles as were suggested and utilized by Kephart (1960,1963) for such children nearly two decades ago. The alternative hypothesis requires the production of no new materials; instead, training procedures aimed at promoting reflection, delaying one's reaction until the alternatives had been considered, would be espoused. Whether or not this particular example has merit, the general point I am making is that both the level of sophistication of the knowledge we desire and the necessary methods to attain this knowledge requires that we pose hypotheses that can shown to lack support. Certainly it is preferable to pit opposing viewpoints against one another which experimental data can distinguish between; such is the genius of great experimental science. At least we should construct experiments which tell us about the failing children's psychological capabilities, a process which will be aided if we insist on validated, i.e., meaningful, instruments, and test alternative hypotheses within a disability group rather than between it and normal controls.

The final research problem I shall address concerns the gulf that exists between much research and the knowledge needed by the practitioner. While it is all but necessary that research will always be seeking knowledge needed by the practitioner, I believe that the methods of research must change if we are ever to confirm a set of principles with attendant data useful to the practitioner. My major argument here concerns the level

of classification that much research appears to have adopted. We have essentially accepted the categories "learning disability" and "minimal brain dysfunction" as appropriate blocking variables for research. As I pointed out in my talk yesterday, research has problems with these labels because they have changed markedly in practice over the years, differ from one locality to another, and generally lack succinct class principles to permit reliable classification. The most basic problem for research here is that incomparability of results can always be posited to lie in the discrepant makeup of the specific samples utilized, thereby undermining the possible accumulation of verified research findings.

In essence I am saying what others have said here yesterday and today; the basic research problem confronting Learning Disabilities (or Minimal Brain Dysfunction) is that of sample definition. I have been concerned with this problem for a number of years during which I have tried to puzzle out its implications for research methodology. The problem essentially is more generic than the study of LD or MBD but concerns instead the creation of a methodology appropriate for the study of deviant psychological functioning.

Because I am going to be speaking about categorization, I would like to take just a minute to address some side issues important to practitioners. There has been a growing emphasis over the years on individualizing instruction, this trend being basic to special education and promoted as a more appropriate means for teaching all children. There are four distinguishable issues here: one is whether the administration of our educational resources should be organized around categories of disability such as blind, deaf, mentally retarded, multiply handicapped, learning disabled, and so forth or whether the funding base should be non-categorical. A second issue is whether the child should be taught as though he were one of

a group of persons representing a certain category of disability, for example perceptually impaired, culturally disadvantaged, and so forth versus whether he should be schooled as an individual, distinct from all other children. This issue has a second facet which I think is important to distinguish. Should the handicapped child in all ways be treated as an individual or are there some aspects of his behavior, notably his handicap, which if recognized as similar to other children's handicaps, could assist us in planning a sound educational program for him? Take a specific example. What comfort would you have in trusting your reading retarded child to a teacher who claimed that because she treated all children as individuals she really didn't know anything about your child's problem and would have to start from scratch and learn how best to deal with his reading problem. Would you not want a teacher who claimed, after an hour or two with your child, to have encountered problems such as his before and exhibit confidence that she were able to deal effectively with problems of that kind? A medical analogy draws this distinction further: when visiting a physician, you certainly do want to be treated as an individual and be afforded all due respect from the physician and his staff but when it comes to the treatment of the disorder that brought you to his office, you would prefer to have the diesase routine in the doctor's experience, one that he has seen many times before, and one that he knows how to treat effectively. Thus, complete individualization of instruction is a two edged sword if such individualization implies that we have learned nothing from past examples of the particular problem a child is experiencing.

I do not think that practitioners hold so naive a position. I think that the practitioner reacts against the category labels used for

administrative purposes such as funding and is arguing that an appropriate education for the child in special need requires consideration of many distinctions not encompassed by such broad terms as learning disabled, minimally brain damaged, culturally deprived, and so forth. At the same time, some level of categorization takes place in the teacher's mind. The experienced teacher is one who has seen a wide variety of educational problems, being able to draw upon her past experience with children having similar problems.

All this may seem quite obvious but when in education circles I hear arguments proposing eliminating categorization and mainstreaming all handicapped children, I wonder whether the issue of administrative categorization is being confused with the natural human tendency to notice similarities within otherwise unique events.

The issues of administrative categories, the question of the child's individuality, and the individualization of instruction are critical for research methodology. Casting these problems as analogous to medical research has been useful for me. From the set of crippling childhood diseases, medical researchers were able to isolate a syndrome which they called polio. Through extensive research, the Salk vaccine was eventually discovered that has proven an exceedingly effective deterent. The fact that individuals contract diseases similar to those contracted by other individuals represents no threat to their individuality. The problems attendant on treating each case and, in fact, the specific ramifications of the disease within any given individual are varied. However, it is still possible to isolate a common core problem that can be named and once named can catalyze further research and knowledge about the disease. It is through the isolation of the common characteristics (syndrome) that we are able to begin amassing knowledge about a disease. In summary,

isolating diseases does not deny the individuality of the people who contract the diseases not does it claim that the disease is exactly the same in all individuals. Naming diseases (diagnostic categorization) does serve a useful function, allowing for the accumulation of knowledge obtained through study of similar cases, and thereby providing us with better methods to treat each individual contracting a sufficiently similar sympton pattern.

I would claim that a similar approach is needed in the area of psychoeducational disabilities. Granting that all children are individuals, there exists behavior patterns that allow us to see some children as more similar to each other then to some other children. One encompassing symptom pattern involves the child's failure to accomplish satisfactory scores on standardized tests or to receive satisfactory grades from the teacher. Such characteristics represent the grossest form of categorization within the educational disability area. The administrative categories used variously in different states such as "learning disabled", "perceptually handicapped", "mentally retarded", "brain damaged", and so forth represent finer distinctions within this initial broad category. It has become clear from many sources and for many reasons that these categories are not very useful; not only are there problems attendant on their use as administrative vehicles; they also lack utility for classroom management and remedial programming. The researcher utilizing the sample difference approach has typically adopted this level of categorization or a level of categorization very similar to it. While adopting category groupings for the study of psychological deviance appears to me to be a very appropriate strategy, the

level of specificity represented by the administrative categories has not been sufficient for the detailed answers being sought in the research as I shall illustrate in a moment. The practitioner needs to be assured that the goal of categorization is not to impune the integrity of the individual but to enable an organized search for lawful relationships between aspects of the child's disability and the conditions which caused that disability and the steps we can take to alleviate it. Knowledge about the causative conditions can give us a preventative outlook while understanding the best remedial procedures can provide substance for our teacher training programs and direction for the individual teacher facing a child with a subborn problem.

What then is wrong with the sample difference approach if, as I to be the case, it has not produced very useful results despite its categorical nature? I think the basic problem is that researchers have focused their attention on the correlates of broad disability categories rather than upon an elucidation of the category system itself. A closer examination of the basic requirements of a category system will allow me to illustrate this point more clearly. Some years ago, Zigler and Phillips (1961) described some of the problems attendant on psychiatric diagnosis. Their conceptualization is useful in the present instance. In studying a specific problem area, such as Learning Disabilities, one needs a set of descriptors known as class principles which can be utilized to determine whether or not a given individual represents an instance of the class "learning disabilities". The more clearly the class principles can be stated, the more reliable should be the determination of whether or not the child belongs in or out of the category "learning disabilities".

If class assignment can be accomplished reliably, then one is in a position to examine the correlates of the class i.e., the variables that relate lawfully to class membership. The sample difference approach is then typically utilized to determine whether individuals sharing class membership also share correlated characteristics of interest such as common causes leading to the problem, common response to various treatments, common underlying process problems such as in attention or memory and so forth.

Within the framework of this research methodology, learning disability research could be expected to encounter problems. Reliability of categorization is a problem because of the changing face of the field of Learning Disabilities throughout the last decade as I described yesterday. Multiple definitions exist, including the medically-oriented definition of Task Force I (Clements, 1966), the Northwestern Conference's "educational" definition (Kass & Myklebust, 1969) both cited in my talk yesterday, as well as a federal legislative definition.[2] None of these definitions have sufficiently clear, objectively stated class principles to permit reliable diagnosis. I do not know of any specific studies on diagnostic reliability but the problems are so analogous to those in the psychiatric literature that I cannot believe that the diagnosis in the educational arena are much superior to those utilizing the psychiatric classification system (DSM-II) which has not proven to be very reliable.

Some of the problems research in learning disabilities faces derive from the inequivalence of research samples caused by the absence of a set of shared class principles with which diagnostic reliability could be established. Without the assurance that samples are comparable, conflicting

results remain uninterpretable. Unreliability restricts the magnitude of the empirical relationships one can find between class memberships and other variable of interest.

While unreliability is certainly a serious problem, it does not totally explain our inability to discover a greater number of important empirical relationships. Heterogeneity of the class membership is frequently cited as the cause of insignificant research findings, one's hypothesis holding true for an insufficiently large subset of the total class to verify the hypothesis statistically. Heterogeneity of class members is assuredly a problem though we must be very clear about what we mean by "heterogeneity". Heterogeneity is variously either synonymous with unreliability of classification or invalidity of class correlates. In the first case, a class could contain members to whom the class principles do not apply, either due to inappropriate assignment or to inadequately specified class principles. The result is a class with members heterogeneous with respect to the class principles, i.e., unreliable.

Alternatively, a class could contain members all correctly classified who still differ on non-class principle variables. Because all persons are unique, all classes containing persons will be heterogeneous with repsect to some variables. Framed in predictive language class membership is an imperfect predictor of most variables and usually an adequate predictor of only a few variables. One hopes that the few variables that are predictable from class membership are theoretically important or useful. The point to recognize here is that all category systems are more or less heterogeneous with respect to class correlates. To bemoan

this fact is only to admit that one cannot locate highly valid class correlates.

Heterogeneity vis-a-vis class correlates, i.e. lack of predictability can be dealt with two basic ways. One is to intensify the search for correlates, concentrating on improving measurement, refining experimental procedures, evolving more potent treatment procedures or even at times looking to new ideas for clues about significant relationships. These steps have been taken. We are just now beginning to examine an alternative approach that of redefining or at least refining the category system itself. In predictive language, this second approach focuses on the predictor rather than on the criteria.

In learning disability research, we do not have predictor classes defined at a conceptual level appropriate to the variables we would like class membership to predict.

Teachers will tell you that the term learning disabilities just as the term mental retardation or the term culturally disadvantaged or the term minimal brain damage are not very useful to them. Typically, the teacher involved with such children must face all of the differences between children within one of these broad categories. To tell her that you as a researcher have found that learning disabled children have an attentional deficit compared to normals which is significant at the .05 level does her no good at all. She wants to know which of the children in her LD class have that attentional deficit, not whether the class as a whole is different from the regular classroom down the hall. This she already knows.

Whether or not the specific researcher is interested in providing data immediately relevant to the classroom teacher, he still wants to

find empirically sound relationships between variables. When studying psychological deviance, the researcher must evolve a set of constructs at an explanatory level appropriate to his line of inquiry. Terms like "learning disability" are quite generic buy may suffice if one is concerned about questions such as the relationship between school problems and (a) self image, (b) delinquent behavior, (c) attendant disruption of the home environment, and so forth. However, if one is interested in the information processing capacities of the learning disabled child and in appropriate remedial techniques which interface with his strengths and circumvent or improve his weaknesses, the category "learning disability" would be an inappropriate choice of sample definition. For example, the class "learning disability" does not appear to be a strong predictor of memory disorders or of attentional disorders though both are certainly represented. Some more carefully defined predictor classes need to be evolved for determining which children will have memory problems and which will have attentional problems. Further refinement in each of these subclasses will likely be necessary to attain high level prediction of other criterion variables of interest. One's theory of educational handicap and attendant research should seek relationships between dimensions of interest (such as learning disability) which may not have as class correlates those variables in which the researcher is most interested. Rather than view the category label as fixed, the domain of behaviors which define the broader category should be the focus of much of the researcher's theorizing and research. We must keep in mind that the creation of a class in the first place represents a belief that there are critical characteristics that

persons so classified have in common and that these commonalities should allow us to predict other important aspects of their behavior. If it turns out that the important aspects that the particular researcher is interested in do not, in fact, lawfully relate to the class, then it certainly behooves him to isolate a different subset of persons for study. It is important to keep in mind that the creation of classes such as learning disability or minimal brain damage are intellectual constructions which may not be an appropriate,i.e. useful, way of looking at the world. Furthermore, it is important to recognize that such classes represent a complex variable which plays a key role in one's theory of educational disability. Unlike constructing a variable which has a number of levels, a taxonomic class asserts that all individuals so assigned are functionally identical. Persons assigned to the class "learning disabilities" do not appear to be functionally identical with respect to the dimensions that are of particular relevance to remedial intervention. In the broader scheme of things, the utilization of classes of individuals which are to be considered functionally equivalent represent an attempt to ignore non-essential differences in favor of recognizing and highlighting essential commonalities. At one extreme, we must recognize that everyone is in fact totally individual i.e., there is some characteristic which distinguishes each individual from any other individual. At the other extreme, one can consider all human organisms as representative of the same class, homo sapiens, the assignment to such a broad class actually telling us many things about the individual, e.g. their dependence on food, water and so forth. The kinds of statements we wish to make about the educationally handicapped child fall somewhere

in between. We can imagine a theory of individual behavior which is so detailed that it is able to predict unique outcomes for each individual. Considerably short of that, we can consider a theory of human functioning which makes predictions about a defined domain of behavior on a level which allows us to ignore some of the differences between individuals resulting in the creation of groups of individuals which are adequately similar for our level of study. Of course, the test of whether they are adequately similar for our purposes is determined by whether we can find variables of interest (class correlates) that lawfully relate to the group of individuals so isolated.

In the area of educational disabilities, we are now facing the fact that research we have done has shown us that the categories we are utilizing (minimal brain dysfunction, learning disabilities, culturally disadvantaged, deaf, blind, and so forth) contain individuals who are dissimilar with respect to the variables we are interested in, i.e. the class correlates which we would have hoped to exist simply do not. What we must do then is search for alternative ways of grouping the children in whom we are interested which may prove more fruitful.

FOOTNOTES

1. This paper derives from a talk presented at the Texas Tech Invitational Conference on the Learning Disabilities Minimal Brain Dysfunction Syndrome: research perspective and applications. October 19-20, 1973.

2. Dr. Senf is Associate Professor of Special Education and Evaluation Research Director for the Leadership Training Institute in Learning Disabilities, College of Education, University of Arizona; on leave of absence from University of Illinois, Chicago where he is Associate Professor of Psychology.

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